The Behavior Analyst 1994, 17, 371–372 No. 2 (Fall)

In Response

Physicalism, Reductionism, and Verbal Behavior: A Reply to Leigland

Nathan Stemmer
Bar-Ilan University

One of the important features of Skinner's program is his physicalism, the view that "an experimental analysis of behavior describes stimuli in the language of physics" (1969, pp. 78–79). This view also covers the analysis of so-called inner events, as expressed for example in Skinner's statements, "My toothache is as physical as my typewriter" (1945, p. 294) and "No special kind of mind stuff is assumed" (1974, p. 220).

Skinner's physicalist treatment of inner events has been criticized not only by cognitivists but also by people who are close to radical behaviorist views (e.g., Killeen, 1984; Natsoulas, 1983) who have argued that certain phenomena, such as the qualitative character of feelings, conscious contents, or states of awareness cannot be accounted for in Skinner's framework. But in Stemmer (1992), I have shown that these criticisms are mistaken. By paying careful attention to Skinner's physicalist position and by explicitly acknowledging the role of inner physiological events in the explanation (but not the prediction) of certain behavioral phenomena, it becomes clear that Skinner is able to account for these phenomena by treating them as physiological events.

In a reply to my paper, Leigland (1993) raises two objections. First, he criticizes my characterization of physicalism according to which all scientifically significant statements are reducible to statements of physics. Leigland argues that this criterion is too narrow for the needs of behavior analysis. Second, he criticizes the alleged reductionist nature of my treatment. Because one of the main features of radical behaviorism is its nonreductionist character—it views a science of behavior as a legitimate enterprise by itself—my treatment would be incompatible with radical behaviorism.

In order to support his first objection, Leigland adduces the results of certain experiments on verbal behavior in which human participants were asked to talk about a particular topic. The subjects were instructed to discover on their own the topic in question, and the experimenter/listener in the adjoining room delivered beeps in the manner of differential reinforcement of successive approximations toward the predesigned topic. The experiments showed significant functional relations. Because the instructions included expressions such as "close to the topic," Leigland observes that the shaping that takes place in such a context "is along a verbal rather than a mechanical response dimension," and he concludes that "no amount of analysis of vocal cord movements or audio spectrographs or other 'physical' measures will be sufficient in characterizing the relevant properties of change in the verbal behavior of the subject" (p. 354).

Leigland is calling our attention to the fact that expressions such as "close to the topic" can be fruitfully used in the analysis of verbal behavior. Experimenters can use them for giving instructions to their subjects and also for reporting their experiments to others. Yet, we are unable to describe in physical terms the circumstances that evoke such responses from speakers of English or the behavior that is controlled by these responses.

I would like to thank Jay Moore for constructive comments on an earlier version of this paper. Address correspondence to Nathan Stemmer, 9 Diskin St., Jerusalem, Israel.

Leigland's conclusion is indeed correct, and it shows that a narrow characterization of physicalism does not adequately express the methodology of radical behaviorism. Rather, as he suggests, pragmatic considerations should be applied for choosing the terminology to be used in behavioral research. This approach does not imply abandoning materialism.

Still, there remains an issue that will have to be addressed eventually. Because the features of the material world that control responses such as "close to the topic" cannot be specified in physical terms, the following question arises: What is the nature of these features, and which are the contingencies that give them their controlling power? But this is not the place to deal with this problem.[1]

Leigland's second objection is that my treatment is reductionist. This claim, however, is mistaken. To be sure, the treatment does introduce inner physiological events. But it explicitly acknowledges our inability to identify the physiological events on the basis of physico-chemical properties. We can only give them an indirect individuation on the basis of functional environment–behavior interactions. Moreover, the events are not supposed to have a predictive function. They play only an explanatory role; they allow us to overcome "embarrassing gaps in our account" (Skinner, 1957, p. 434). There is therefore no question at all of an investigation of physiological events to replace the investigation of behavioral phenomena. Consequently, my treatment is clearly nonreductionist (as stated on p. 117 of Stemmer, 1992).

Still, because my characterization of physicalism speaks of statements that are *reducible* to statements of physics, the characterization may have been misleading. I therefore take this opportunity to clarify that it was meant to imply that scientific statements should *refer* to physical features of the world, and the reason for this restriction was to avoid all reference to nonphysical "mental stuff."

In conclusion, Leigland is right that my characterization of physicalism does not agree with radical behaviorism. Pragmatic considerations must determine the terminology to be used, but no nonmaterial mental stuff should be introduced. On the other hand, his second objection is incorrect. By admitting its inability to specify the physiological events in physico-chemical terms and by refusing to assign predictive power to the events, my treatment is not reductionist.

[1] Let me only briefly mention the view that is based on Quine (1960, 1974) and that is developed in more detail in Stemmer (1983, pp. 138–150). Expressions such as "topic" are normally learned in *intraverbal contingencies*. That is to say, and restricting ourselves to listener behavior, the members of a verbal community learn an intraverbal by hearing it within a verbal context that is already known to them (see, e.g., Skinner's account of the intraverbal learning of the word "amphora," 1957, p. 360). The known verbal context has been learned previously, perhaps again intraverbally with the help of a known verbal context. But because there is no infinite regress, the "final" context words have been learned by a direct confrontation with environmental stimuli, because only such learning processes require no previously known verbal context. Now, according to the above view, the features of the material world that control an intraverbal are derived in a complex (and largely unknown) manner from the "final" environmental stimuli.

REFERENCES

Killeen, P. R. (1984). Emergent behaviorism. *Behaviorism, 12,* 25–39.

Leigland, S. (1993). In response: The case against physicalism in the analysis of behavior. *The Behavior Analyst, 16,* 351–355.

Natsoulas, T. (1983). Perhaps the most difficult problem faced by behaviorism. *Behaviorism, 11,* 1–26.

Quine, W. V. (1960). *Word and object.* Cambridge, MA: MIT Press.

Quine, W. V. (1974). *The roots of reference.* La Salle, IL: Open Court.

Skinner, B. F. (1945). The operational analysis of psychological terms. *Psychological Review, 52,* 270–277.

Skinner, B. F. (1957). *Verbal behavior.* New York: Appleton-Century-Crofts.

Skinner, B. F. (1969). *Contingencies of reinforcement: A theoretical analysis.* New York: Appleton-Century-Crofts.

Skinner, B. F. (1974). *About behaviorism.* New York: Knopf.

Stemmer, N. (1983). *The roots of knowledge.* New York: St. Martin's Press.

Stemmer, N. (1992). Skinner and a solution to the problem of inner states. *The Behavior Analyst, 15,* 115–128.